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ANNEX



AN
INQUIRY
INTO THE
MODUS OPERANDI
OF
MEDICINES
UPON THE
HUMAN BODY.

TO WHICH ARE ADDED,
SOME OBSERVATIONS
ON THE
ACTION OF THE LYMPHATICS.

BY WILLIAM WYATT BIBB, LIBRARY 21371
OF GEORGIA;

Member of the Philadelphia Medical and Chemical Societies.

“What mischief have we done under the belief of false facts (if I may be allowed the expression) and false theories!—We have assisted in multiplying diseases.—We have done more—we have encreated their mortality. I shall not pause to beg pardon of the faculty for acknowledging in this public manner the weaknesses of our profession. I am pursuing truth—and while I keep my eye fixed upon my guide, I am indifferent whither I am led provided she is my leader.”

RUSH'S INQUIRIES, vol. 1.

PHILADELPHIA:
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1801.

AN
INAUGURAL DISSERTATION
FOR THE DEGREE OF
DOCTOR OF MEDICINE,
SUBMITTED TO THE EXAMINATION
OF THE
REV. JOHN EWING, S. S. T. P. PROVOST;
THE
TRUSTEES AND MEDICAL FACULTY
OF THE
UNIVERSITY OF PENNSYLVANIA,
ON THE
EIGHTH DAY OF JUNE, 1801.

TO
DOCTOR JOHN MURRAY,
OF
AUGUSTA, GEORGIA.

DEAR SIR,

TO pass over in silence the numerous advantages I have derived from you, in the two-fold character of a friend and learned preceptor, when I have an opportunity of making a public acknowledgment, would be deviating too far from that propriety, which justice and gratitude prescribe.

Permit me to declare that under your care I commenced my medical pursuits, and that your attention towards me, and exertions for my future prosperity, have been far greater than a pupil had a right to expect. Be assured, Sir, that the many favours you have conferred upon me shall ever be held in grateful remembrance.

Please accept a dedication of this my first essay as a small, but sincere mark of the high regard I entertain for your person and talents.

Wishing you the constant enjoyment of happiness and prosperity through life,

I am Sir,

Your Sincere Friend,

And former Pupil,

WILLIAM WYATT BIBB.

TO

BENJAMIN RUSH, M. D.

*Professor of the Institutes, Practice, and of
Clinical Medicine,*

IN THE UNIVERSITY OF PENNSYLVANIA.

DEAR SIR,

IN dedicating to you this imperfect Essay, I am influenced by a desire to express the high esteem I entertain for your person and distinguished talents; to declare that many of my medical principles I have the honour to derive from you; and, to return my sincere thanks for the valuable instruction you have afforded me.

That your unrivalled abilities, and your exertions in the cause of humanity, may elevate you above the reach of the calumnies and unmerited persecutions which you have so often sustained, and that your eminent services may obtain the permanent remembrance of a grateful people, is the ardent wish of

Your friend

And fellow citizen,

WILLIAM WYATT BIBB.

PREFACE.

Apologies, by way of preface, are so common, that it is with some reluctance I attempt to offer one. But duty to myself commands me to premise, that the intricacy of my present inquiry, the want of a pilot, and the short time allotted for writing an Inaugural Essay, combine with my very early youth and inexperience, to prevent the following pages from being so worthy public inspection, as I could wish. I am, however, compelled to obtrude them, claiming that indulgence which the preceding considerations may be thought to merit.

AN INQUIRY, &c.

THE subject, upon which I am about to enter, is one of the most comprehensive, and intricate, I believe, in the science of medicine. It is one, than which, in many of its essential relations, none can be of more importance, and, I may add, none is more involved in doubt and obscurity.

In commencing this enquiry I am immediately met by a question, to solve which in a satisfactory manner, I confess my inability—This acknowledgment, I presume, will accord with that of every candid physician. The question to which I allude is this—On what do medicines act when applied to the human body? We know they act upon the vital principle—but what is life, how formed, and how attached to the animal body, are questions, an explanatory solution of which is either reserved for a more enlightened age, or will forever

be united with the important mysteries of nature.

Without then offering a discussion of the various opinions, relating to the form, action, and obscure laws of life, I shall content myself with observing that it is the principle upon which impressions are made, that it is the source from which actions are derived, and that it is by it and stimuli, all the operations of the animal body are performed. I proceed then to enquire into the operation of medicines upon the human system.

The human body is so constructed, that although there is an intimate connection between its various parts, although one sound part often sympathises with another that is diseased, and although medicines frequently operate upon one part through the medium of another, yet we are not to infer that one of these parts cannot be affected by disease or medicine without the whole: Such an inference would be erroneous, as I hope to prove hereafter, inasmuch as it is contradicted by the established laws of diseases, and the operation of medicines employed for their cure.

It is my intention in this essay to evince that all medicines act specifically* upon one

* As the word *specific* is frequently employed in the fol-

or more parts of the body—that they are all stimulants†; and that their various effects which have induced writers to divide the *matéria medica* in stimulants, tonics, astringents, &c. arise entirely from a stimulant *specific* operation, exerted upon their appropriate parts of the animal system.

Believing every disease, to which the body is subject, to have a particular seat, and every medicine to exert a *specific* operation, I think myself supported by the laws of nature, in dividing the general system into several lesser ones. Directed then by the effects of diseases, and the operation of medicines, I shall divide the body into six systems, viz. 1st. the Visceral System, in which I include the stomach and intestines. 2d. The Sanguiferous System. 3d. The Nervous System, including the brain and nerves. 4th. The Muscular. 5th. The Absorbent, and 6th. The Glandular System.

lowing pages, and as it has often been used indefinitely, it is necessary I should define what is meant by it. I mean then, that every medicine operates in a way peculiar to itself, and acts, in whatever manner applied, on particular parts chiefly.

† In asserting that all medicines are stimulants, I only include medicines properly so called. The abstraction of stimuli from the body, is not considered as entitled to that appellation.

The relation which these different systems bear to each other is very unequal. The connection of the stomach with all of them is far more intimate than that which subsists between any others. The different parts of the body, therefore, are very liable to be affected, through the medium of this viscus, both by diseases and medicines, and vice versa.

I am aware that the possibility of any medicine operating specifically on one part, without exerting the same action in other parts, has been denied, and this denial is grounded upon the supposed sameness in the operation of all medicines, and the peculiar sympathy which connects the different component parts of the body. I acknowledge that a superficial view of the organization of the whole system, and of the uniform order of its various actions in a healthy state, renders such an opinion plausible. But a more minute examination into the causes of those actions, into the operation of the various stimuli which support life, and into the effects of diseases and medicines, upon this organized body, dissipate at once this seeming plausibility, and establish on the firm basis of fact and observation the propriety of the doctrine I advocate.

Such, indeed, is the independency of each system, and such the difference in the operation of the same causes upon them, that certain applications made to one, will be productive of fatal effects, which to another will be attended with mild and salutary consequences. To exemplify this position it is only necessary to adduce the action of the carbonic acid. This, when inhaled into the lungs in form of gas, produces deleterious effects, yet it may be taken into the stomach, combined with water not only with safety, but with pleasure. In this way it is so mild and agreeable, that many persons use it in the heat of summer as a pleasant diluting drink. I know it has been denied that carbonic acid gas, operates positively upon the lungs, and on the contrary asserted, that its fatal effects arise only by depriving them of atmospheric air. But this assertion is unsupported by facts and observation. History informs us, says Chaptal, that two slaves whom Tiberius caused to descend into the Grotto Del Cano (which abounds with this gas) immediately expired. The same fate attended two criminals, whom Peter de Toledo ordered to be shut in there. Many similar instances are upon record, of immediate death from carbonic acid in old

cellars and wells. It is said that the famous lake of Averno, where Virgil placed the entrance of hell, prevents birds from flying over it with impunity, owing to the exhalation of this gas. The experiments of chemists fully prove its positive action. It is a notorious fact, that we can voluntarily stop breathing without much inconvenience, for a longer time than is required for this poison to produce its baneful consequences.

The matter of syphilis which has done so much injury to mankind has been taken into the stomach without producing any of those affections consequent upon its application to its appropriate parts.

Dr. Fontana informs us that the poison of the viper is perfectly innocent when taken into the stomach, but when infused into a wound, produces the most terrific consequences.

The foregoing are certainly examples not only of the specific operation of medicines upon particular parts, but of their directly opposite effects; evincing in a decisive manner the distinctness and independency of the different systems.

As the investigation of this subject is not a matter of mere idle curiosity, but of the utmost practical importance, I shall pursue the

operation of medicines, with a view to shew their *specific* action upon the different systems; and first

UPON THE VISCERAL SYSTEM.

Although I have divided the body into six systems, yet there are some medicines which operate principally upon only a part of a system; an example of which is afforded by the stomach and bowels—thus the tartrate of antimony acts upon the former and jalap on the latter.

The unusual sympathy which I have declared to exist between the stomach and every part of the body; it being the medium through which they are acted upon by medicines, and often partaking of the diseases of either system, necessarily induces a belief that if medicines can operate specifically upon this important viscus, the same operation may attend their application to any other parts. That there are *specifics* for the stomach is proved by the action of emetics, and that there are the same for the intestines is equally evinced by the operation of cathartics. If those substances which induce vomiting or purging

when applied to the stomach or bowels, be applied even to distant parts they will produce the same effects. In proof of this assertion many facts may be collected. Mr. Sherwin rubbed four grains of tartrate of antimony into the palms of his hands, which in a short time produced vomiting. The same effect has resulted from injecting it into the blood vessels. Mr. Smith injected a laxative medicine into the median vein of a patient in the hospital of Dantzic, which in four hours commenced a purgative operation continuing until the next day; and the celebrated Borrichius mentions that hellebore placed in an issue in the arm produced violent purging.

Those substances which excite sneezing when snuffed up the nose, will produce the same effect if taken into the stomach. Professor Barton informs us, in his lectures, that the honey which the bees prepare from the flowers of the *kalmia latifolia*, when taken into the stomach, causes sneezing.

The powder of jalap or rhubarb when swallowed, passes through the stomach without much effect, but when they arrive to the bowels their chosen parts, they commence a powerful cathartic operation. This is not the case with ipecacuanha or the tartrate of

antimony which acts on the stomach, causing that violent exertion we term vomiting. How can this difference in the effects of these different substances be explained? and how shall we account for certain medicines affecting particular parts, although applied to distant ones?

If all medicines are stimulants, and their operation precisely the same, varying only in degree, as is asserted by some writers, their effects on the system should also be the same, varying only in degree. But we have seen that different medicines do operate particularly upon different parts, and produce effects entirely different; therefore we must explain the fact in some other way.

This is easily and plainly done by admitting the evidence of truth which indubitably declares, that all medicines have a *specific* action upon particular parts of the animal body.

In compliance with the opinion which supposes the only difference in medicines to consist in the degree of stimulant property, every substance which would excite purging must produce vomiting in its passage through the stomach; because this viscus is endowed with more excitability or a greater aptitude to be

acted upon by stimuli than the intestines; therefore, those medicines which produce any action in the latter, must excite much greater action in the former. The contrary of this, experience teaches us to be true, consequently a *specific* operation must be acknowledged.

Colombo, gentian, and bitters in general act specifically upon the Visceral System.— With a view to ascertain the propriety of this position, I instituted the following experiments.

Experiment 1.

To my friend Mr. Mitchell, at 50 minutes after 9 o'clock, his pulse beating 70 strokes in a minute, I gave 20 grains of powdered gentian, mixed in a little milk. His pulse was examined every five minutes attentively, for half an hour, without any variation being perceived. The same quantity was now repeated, and the examinations were continued with the same result, until 15 minutes had expired, when his pulse beat only 67, in 5 minutes after 66, in 10—66, and in 15—64. The dose was again repeated, in 5 minutes his pulse beat 64, in 10—64, in 15—63, 20—63, 25—62, 30—62, 35—63, 40—64, 45—61, 50—58, 55—58 and in 60—60. The heat of his

body was a little increased, his pulse appeared to be somewhat fuller, and he complained of a pain in his head.

Experiment 2.

To my friend Mr. Lockette, at 10 minutes after 10 o'clock, I gave 20 grains of powdered gentian as above, his pulse beating 82 strokes in a minute—in 5 minutes it beat 82, in 10 82, in 15—81, 20—78, 25—77, and in 30—76. The same quantity was again exhibited, in 5 minutes his pulse beat 76, in 10—77, 15—77, 20—77, 25—73, 30—71, 35—70, 40—71, 45—73, 50—73, 55—73, and in 60—73. No other alteration in the pulse was observable. He felt some degree of head-ach.

Experiment 3.

At 45 minutes after 9 o'clock, my friend Mr. Jackson, his pulse beating 78 strokes in a minute, took 30 grains of powdered columbo, mixed in a little milk, some degree of nausea ensued; in 5 minutes his pulse beat only 68, and very weak, in 10—73, in 15—74, 20—75, 25—75, and in 30—77. The same quantity was now repeated, in 5 minutes his pulse beat 73, in 10—73, 15—76, 20—76, 25—76, and

in 30—76. The same dose was again taken, in 5 minutes it beat 73, in 10—75, 15—77, 20—74, 25—70, 30—71, 35—72, 40—70, 45—70, and in 50—70. In the latter part of the experiment, the pulse was somewhat increased in force and fullness.

Experiment 4.

At 20 minutes after 10 o'clock, my friend Mr. Shepperdson, took 30 grains of columbo as above; his pulse beating 68 strokes in a minute. In 5 minutes it beat only 63 and very weak; in 10—63, 15—61, 20---60 25—59, and in 30---59. The dose was repeated. In 5 minutes his pulse beat 59—in 10—59, 15—58, 20—57, 25---57, and in 30, 58. The same quantity was again taken; in five minutes 58, in 10—58, 15—58, 20---60, 25—57, 30—57, 35—56, 40—55, 45---55, 50—54, 55—53, 60—53, 65—53, 70—53, 75—53, 80—55, 85—55, 90—55, 95---55, and in 100---55. His pulse increased a little in fullness and force towards the latter part of the experiment, and he complained of a peculiar sensation in the stomach which he could not describe.

The foregoing experiments fully evince 1st. That columbo and gentian do not stimu-

late the Sanguiferous System; therefore, that the generally received opinion respecting their operation is highly erroneous, and has had an injurious tendency in the practice of medicine.

2d. That they act *specifically* upon the Visceral System, increasing its action, and thereby lessening the action of the heart and arteries---and

3d. That they may be exhibited with perfect safety in diseases of the stomach and bowels, without any regard to the state of the blood-vessels.

From these facts I am induced to believe that beneficent nature has among her various productions, remedies peculiarly adapted to the cure of the diseases of every system of the body; and that medical science has, and will derive its greatest advantages from the discovery of them, and the proper application of the principle of *specific* actions to the cure of diseases.

II. THE SANGUIFEROUS SYSTEM.

Let us next examine whether there are not medicines which operate particularly upon this system.

That the peruvian bark is a *specific* for this system, is rendered evident by its successful application in the cure of its diseases.

It may however be said, that any medicine equally stimulating with the bark, will be equally efficacious in the cure of fever, and that its effects do not prove a *specific* operation. This however, is far from being true, for opium which stands at the head of stimulants in the materia medica, is acknowledged by most physicians to be far less successful. The bark may be employed in cases with advantage in which opium would be productive of the worst consequences; the reason of which is, that the stimulant operation of the former is principally confined to the blood vessels, while the latter acts as a potent stimulus upon the whole system. It is by this general operation upon the whole body, producing that degree of debility in every part, which Doctor Rush has called the sleeping point, that opium so much more uniformly induces sleep than other medicines. The fol-

lowing experiment will evince the propriety of my opinion as to the operation of the peruvian bark.

To my obliging friend Mr. Little, at forty-five minutes after eleven o'clock, having breakfasted at eight, I gave half a drachm of best red bark, mixed in a little milk, his pulse beat seventy strokes in a minute. I examined it every five minutes attentively until a quarter after twelve, without perceiving the smallest variation in force or frequency. I then gave him a drachm in the same form, repeating my examinations with the same result, until thirty-five minutes after the same hour, when it beat seventy one. The dose was now repeated, in 5 minutes it beat 73, in 10—74, in 15—75, 20—76, 25—77, his face very much flushed, and he complained of pain in his head. I now gave him the same quantity as before, in 10 minutes his pulse beat 78, and in 20—81. He now took the last dose, in 10—85, and in 20—87, above which it did not rise. There were no other perceptible effects produced, except a small degree of nausea.

The experiment now related tends to prove, 1st. That, the action of the peruvian bark is confined principally to the blood-vessels, and that, for this system it is a powerful *specific*

stimulant ; 2dly. That, the human body is so constructed, that one system is capable of receiving powerful impressions even through the medium of other parts, without those impressions being communicated to the whole ; and 3dly. That, from the early commencement of its operation, after it was exhibited, it may produce its beneficial effects in the cure of fever through the medium of the stomach.



III. THE NERVOUS SYSTEM.

The operation of medicines, upon this system, next merits our attention.

That every medicine has its appropriate system or systems, I am clearly convinced. I say systems, because some remedies act only on one, whilst others act upon more. Among those which operate upon this system, the fœtid gums hold an eminent station ; assafoetida is one, the exhibition of which in nervous diseases, has ever been attended with the most salutary effects. But alas ! physicians not deducing proper conclusions from the experienced donations of nature---not viewing with propriety her beneficent operations, and forgetful that she employs different means to ac-

comply with her various purposes, have been led to conclude, that this medicine may be applied with equal advantage in the cure of the diseases of the blood-vessels---As well might we attempt to cure hypocondriasis by blood-letting, because it is efficacious in the removal of fever. Assafœtida has been recommended in the low state of fever, when the most potent stimuli are required. And, those who use it in these cases, expect to obtain all the advantages of a powerful stimulus. That it is a powerful *specific* stimulus for the Nervous System, I agree, but that it has no perceptible operation upon the blood-vessels, will be proved by the following experiment :

To the same friend on whom I performed my former experiment, I gave at half past eleven o'clock, six grains of assafœtida, in the form of a pill, his pulse beat sixty strokes in a minute. The same quantity was repeated every half hour, until he had taken twenty-four grains, when after the same interval I gave him double the quantity.

The whole amounted to thirty-six grains in the space of two hours, which is more I believe than is usually exhibited in cases of disease. His pulse was examined every five minutes attentively, but without perceiving

the least variation either in force or frequency. The only perceptible effects were the following: he was much more lively than usual, and felt his mind exhilarated, which was succeeded by nausea and flatulency. The smell of the gum was evident in his urine the next morning.

This experiment plainly shews the extreme error into which physicians have fallen who recommend the fœtid gum for the cure of the diseases of the Sanguiferous System. Such a practice originating from the supposed sameness in the operation of all stimulants, has certainly done much mischief: because, as I have shewn that assafœtida does not produce any perceptible effect upon the blood-vessels, surely to trust to it in diseases of this system is almost leaving the patient to the operations of nature. Here then we discover the immense advantage which must result to the science of medicine from investigating the action of medicines upon the separate systems of the human body. When that time arrives, at which the subject shall be so far developed, that the diseases of every system shall have their appropriate remedies, then will epilepsy cease to be incurable, then will all those diseases which now elude the skill of the medical

world, become curable, and then will the science of medicine reign triumphant throughout the civilized nations of the earth.



IV. THE MUSCULAR SYSTEM.

This system is so nearly allied to the nervous, that the majority of those medicines which act particularly on one, exert also a *specific* operation upon the other. I have, however, thought proper to divide them, from a belief that there are some medicines whose action is more confined to one than the other. The oil of amber, I consider a *specific* for the Muscular System. This consideration is corroborated by its successful application in the cure of tetanus, and similar diseases. Dr. Rush observes, that he has used this medicine with obvious advantage in tetanus, and thinks it well adapted to the diseases of the Muscular System. Dr. Cullen admits the successful exhibition of the “fœtids” in the cure of spasmodic diseases, but accounts for their operation in the following manner: “The operation of these I take to be in this way,—“that as all disagreeable sensations are sedative, or means of weakening the energy of

“ the brain, so I conceive that our fœtid medicines, by obviating or moderating the increased excitement which begins spasmodic affections, may be the remedies of these.”

It will be readily perceived, that the Doctor commences his explanation of the *modus operandi* of the fœtid medicines upon a foundation which has no existence in truth ; consequently his conclusions must be involved in error. That muscular diseases often arise from, and are connected with morbid nervous affections, is true ; but that they are not always the effect of morbid excitement in the brain, is proved by the fact, that tetanus and convulsions are almost always preceded by, and connected with, the proper exercise of all the functions of the brain, the operations of the mind being perfectly unimpaired.

As to the Doctor's opinion of the sedative action of the “ fœtids,” I deem it unnecessary to spend any time in controverting it. I shall only observe that my experiment with the *assafoetida*, which I have related, evidently evinced a stimulant action. Our author has attributed the same sedative operation to opium, and all the most potent stimulants in the *materia medica* ; and, although he does this, we find him employing them in typhus fever,

and in all cases of great debility, where stimulants are required ; at the same time forbidding their use in inflammatory affections. Here we have a specimen of the agreement between the theory and practice of the late Edinburgh Professor.

As I have shewn that the fœtid gum operates as a powerful stimulant upon the Nervous System, and as I shall relate an experiment immediately, which proves the same operation of the oil of amber upon the muscles, we must offer an explanation of their *modus operandi* in the cure of diseases of these systems, very different from that of Dr. Cullen. I suppose then their good effects to arise from a *specific* stimulant operation of each, exerted upon their appropriate systems, overcoming and destroying the morbid action, and producing a new and healthy one. With a view of ascertaining the action of the *oleum succini* upon the human body, I made the following experiment :

To my friend Mr. Jackson, at eleven o'clock (his pulse beating seventy-five strokes in a minute) I gave ten drops of the rectified oil of amber mixed with a little sugar. This dose was repeated every fifteen minutes, until he took four times that quantity. His pulse was

examined every five minutes attentively, without perceiving the smallest alteration until after the exhibition of the last, when it appeared to be a little fuller, without, however, any increase in quickness or frequency. After the same interval he took twenty drops, which were again repeated---his pulse the same.--- In five minutes it beat 74 only, in 10—73, in 15—73, in 20—72, in 25—71, in 35—71, 40—71, 45—71, 50—72, and continued to rise from this time until it arrived to its original standard, but did not exceed it. The operation of the medicine upon the muscles and brain was very perceptible. His mind was exhilarated, and he complained of languor or a painful difficulty in exerting his muscles. At half past two, he walked out but returned in a short time, finding the exercise of his muscles unpleasant.

I was assisted in the foregoing experiment by a physician who believed the only difference in medicines to consist in their degree of stimulant property. The result was as I have stated. Why did the pulse diminish in frequency and increase in fullness---and why were the muscles so debilitated? These questions may be answered together, as I believe one to be dependant upon the other.---

The oil of amber exerted a powerful *specific* stimulant operation upon the Muscular System, producing great indirect debility. This accounts for the painful exercise of the muscles. The diminution in the frequency of the pulse was caused by this stimulant operation on the Muscular System, increasing action there, and thereby lessening the action of the heart and arteries. It may be asked why increasing action in one part should diminish it in another? I answer, I know not, but such is the fact; we must therefore admit it, and refer it to a law of the animal economy.



V. THE ABSORBENT SYSTEM.

This is so very important in the animal body, and a constant exercise of its functions so essential to health, that, to deny the existence of *specific* remedies for its diseases, would be accusing nature of improper partiality.

The preparations of mercury and iron act powerfully upon this system. Digitalis and squills may likewise be said, with propriety, to operate upon the absorbents. The justness of this opinion is fully evinced by their successful application in the cure of anasarca.

The great discharge of urine, accompanied with a sudden diminution of the size of the abdomen, which sometimes occurs in ascites from the exhibition of digitalis, cannot be explained in any other way, but by admitting a *specific* operation upon the absorbents, causing them to absorb the effused fluid, and upon the kidneys producing an increased secretion of urine.

It is well known that digitalis will lessen the action of the pulse, the conclusion from thence has been, that it operates powerfully upon the blood-vessels; hence some have supposed it a sedative, whilst others declare that it prostrates the action of the arteries by its powerful stimulant action upon them.

Both of these opinions I believe to be erroneous, as this medicine produces no action in the Sanguiferous System, but what arises from its *specific* operation upon other parts. It acts specifically upon the absorbents and kidneys, increasing their action, thereby lessening the action of the blood-vessels. Thus, morbid action in the ligaments may be overcome by exciting action in the skin by cantharides, or any other irritating application; mania and consumption often cure each other. If nux vomica be given to a dog, and he be

whipped, it will not affect him. Many similar instances, of action in one part destroying it in another, might be adduced, but I deem it unnecessary.

It may with truth be observed, that those systems of the body, the proper *specific* medicines for which have been discovered, are far more easily cured of their diseases, than those the important remedies for which are yet unfortunately concealed. Thus, intermittent fever is nearly uniformly curable—Why? Because, peruvian bark, a powerful *specific* for the Sanguiferous System is discovered. Syphilis, also, is nearly always to be cured—and why? Because, mercury, the destined *specific* for the Glandular System, happily for mankind, is generally known. Nothing is wanting to render diseases of the other systems equally curable, but equal progress in the discovery of their *specifics*.

VI. THE GLANDULAR SYSTEM.

This has also its *specific* remedies. The Glandular System, however, is more frequently acted upon partially, by different stimuli, than any other of the body. It is so extensive, and the different glands which compose it so distantly related, that many of them which are nearly connected by contiguity, or similarity of structure, are alone affected by particular medicines. Thus, digitalis and nitre operate upon the kidneys, yet they have no sensible effect upon the salivary glands or the liver.

Some of the preparations of antimony and sulphur act upon the glands of the skin, yet they do not operate upon many other parts of this system. The *specific* operation of cantharides upon the glands, on the internal surface of the neck of the bladder, is fully evinced by the fact that they will often affect them, although applied to the most distant parts of the body.—Marsh miasmata exerts a *specific* operation upon the liver, though not upon other glands. Even mercury whose operation upon the glands is more general than any other medicine with which I am ac-

quainted, acts far more powerfully upon the salivary glands and liver, than upon the other parts of this system. That, mercury operates specifically upon the salivary glands, although it acts upon other parts of the body, cannot consistently with truth, be denied. I know there are cases upon record of other medicines salivating, but in these instances the action of the glands which secrete the saliva, is very different from that which mercury excites. Tobacco, and many other acrid stimulants when held in the mouth increase the salivary discharge, but this is not attended with that peculiar fœtor, fore mouth, &c. which uniformly distinguishes the operation of mercury from that of every other medicine.

Ever since the discovery of the salutary effects of this mineral in the cure of syphilis, which is a disease of the Glandular System, physicians have been in constant controversy relative to its *modus operandi*. To enumerate the various opinions which have been entertained upon the subject, and the arguments by which they have been supported, would require far greater limits than I have prefixed to this essay. And I confess I am by no means anxious to present such a collec-

tion, as it would be only exhibiting a picture of the late degraded state of medical science. I therefore proceed to offer the opinion I have adopted upon the subject.

The matter of syphilis when taken into the human body, acts as a powerful stimulant, particularly upon the Glandular System, producing there its peculiar effects. That, mercury operates specifically upon the same system, cannot reasonably be denied. Now, as no two actions can exist at one and the same time, in the same part, when mercury and syphilitic matter are both applied, one must succeed in destroying the action of the other. Experience teaches, that the former is most powerful, therefore eradicates the venereal action, by a superior *specific* stimulant power, and either establishes its own action which speedily terminates in health, or immediately produces a new and healthy one.

It has been asked why opium, stramonium and other medicines, generally considered far more stimulating than mercury, are not equally successful in the cure of syphilis. To this I would reply, that although the operation of these medicines upon the whole body, is more powerful than that of mercury,

yet I am supported by the experience of physicians in asserting, that this latter has a more potent stimulant *specific* action upon the individual Glandular System, which is the seat of disease, than any other medicine in the materia medica. I suppose its superior efficacy, then, to consist in the superior property now mentioned.

That almost incurable disease called cancer is seated in this system. Although mercury and cicuta operate so strongly upon it, yet the cancerous action in most cases is so great, that they cannot overcome it. It is, however, certain, that the most advantage is derived from the application of those medicines, which act specifically upon the Glandular System. Is there not a *specific* sufficiently powerful to overcome this fatal action? There certainly is: but until it is discovered, many of our unfortunate fellow-creatures must fall victims to its ravages.

I am supported then by truth, in deducing the following conclusions.

1st. The different diseases of each system are curable, or incurable, according to the progress which has been made in discovering their *specifics*.

2d. The only method by which we can arrive at certainty, in curing diseases, is, by attentively investigating the operation of medicines upon the separate systems of the body.

The doctrine of *specifics* which I have delivered leads to the most important objects in the practice of medicine. 1st. To a proper selection of remedies for curing the diseases of every system, whereby we are capable of acting upon the diseased part, without much affecting the healthy ones. 2dly. To a proper selection of medicines, to excite new action in parts less essential to life, thereby, abstracting morbid action from vital ones. These then being objects of the greatest magnitude, it is our duty to adopt every consideration which may, in the smallest degree promote their accomplishment. Nothing, in my opinion, will so certainly produce this effect as a well directed attention to the partial operation of medicines, and a judicious arrangement of the different articles of the materia medica according to the systems, or parts of systems, upon which they are found to operate. The advantages that would result from such an arrangement, over every other hitherto proposed, are many and important.

1st. According to the hitherto adopted division into stimulants, astringents, tonics, &c. we are incapable of selecting with any degree of certainty such medicines as are adapted to the cure of the diseases of a particular system. Is a patient labouring under tetanus? It is a common, and I believe a proper practice, to employ stimulants in most cases. Now, by what are we to be governed in our selection from this numerous class? By nothing, as far as I know, but the peculiar whim of the physician; hence has arisen the exhibition of such a vast variety of unappropriate medicines for the cure of every disease.

But, according to the arrangement I have proposed, we have fixed laws to direct us. The first circumstance to be ascertained, before we prescribe for a patient, is the system or systems diseased, and the nature and grade of morbid action; then by referring to that class of medicines which is appropriated for the diseases of those parts, which are now affected, we can at once select the proper remedies.

2d. A proper examination of the operation of medicines, upon the separate systems, is the only proper method of ascertaining their properties. If, in exhibiting them, we

direct our attention only to their effects upon the whole system as one indivisible machine, their operation upon the different parts may easily pass unnoticed, but by minutely observing their action upon the separate systems, we shall discover the parts upon which they act specifically, and profit accordingly.

Having finished my intended observations upon the *specific* action of medicines particularly, it is necessary I should next enquire how they operate, whether through the medium of the stomach or the circulating blood.

That all medicines act through the medium of the stomach alone, has never, I believe, been asserted by any writer, but this opinion is now entertained by some physicians of eminence. It is my opinion that they operate most frequently in this way, but that they are sometimes absorbed in their active state, and produce their peculiar effects through the medium of the circulating blood. I shall attempt to prove the propriety of my opinion, by the introduction of positive facts, which theoretical speculations cannot controvert.— I have heard them denied, although derived from the first authority; because, similar facts have not occurred to many other writers. But, the man who offers this denial, acts as

unphilosophically as I should, were I to deny the existence of any thing, because I never saw it. It is an ancient, and true axiom that negative proof is no proof, when opposed by positive; therefore, one positive fact will overbalance a thousand negative ones. If then the opinion I hold, is supported by positive proof, and opposed only by negative, I shall consider myself certainly supporting that side of the question in which propriety resides.

It is now my object to prove that medicines have been found in different parts of the body, after having been taken into the stomach and passed through the circulation.

The great quantity of blood which circulates through the human body, and the small proportionate quantity of medicines which is exhibited, render an attempt to detect them in this fluid difficult, and even impossible I believe, in common cases. The only instances in which they can be found here, are when they possess much colouring principle, or when they have been used for a great length of time. Madder tinges the serum of the blood of a red colour, and Doctor Prestwich speaks of a case, in which globules of mercury were found in this fluid. As it is much

more easy to detect the presence of medicines in the secretions, than in the whole mass of blood, so we have a greater number of instances of their being detected here.

Doctor Hamilton of Edinburgh, detected globules of mercury in the milk of a nurse under salivation, by slow evaporation. The milk of women frequently possesses the entire properties of the medicines they have taken. Thus we are credibly informed, that the milk of a nurse in a state of salivation will salivate the child, and every old woman knows that cabbage eaten by the mother, will gripe the infant. Professor Barton, mentions several instances of opium, ardent spirits, and cathartic medicines, producing their usual operation upon children, through the medium of the mother's milk, although in some the mother was not the least affected. Doctor Percival also observes, that a purgative given to one who suckles, will sometimes produce no operation upon her bowels, but a powerful one on the child at the breast. I am aware it may be urged that the effects produced upon children in the preceding instances by the mother's milk, do not arise from the presence of medicines in it, but from their action upon the mother through the medium of the

stomach, producing disordered actions in her system, and thereby creating a morbid secretion. In support of this idea may be adduced the fact, that, if an angry mother suckles her child, it will be often violently affected. To this I would reply, that the fact is no argument against the opinion, which supposes the presence of medicines in the milk, that were taken into the stomach; because in the cases related by Drs. Barton and Percival, the medicines had no operation upon the mothers, therefore there could be no cause for a morbid secretion. Besides, the milk of an angry mother has no determinate effect upon the infant, whereas that of a woman who has taken medicine produces the same effects precisely as the medicine itself. Thus, has the mother taken jalap? her child is purged; has she taken opium? her infant is put to sleep—and if ardent spirits, it is intoxicated.

Professor Thunburg, having used lead for some time internally, perceived it in his saliva. Mr. John Hunter, whose authority is equal to that of any writer, found all the preparations of mercury soluble in the saliva, and with a view to ascertain, whether the same taste would be produced in his mouth by applying them to distant parts which he perceived by

holding them there, he made the following experiments. He rubbed mercurial ointment into the internal part of his thighs, until his mouth became affected, when he plainly tasted the mercury. After recovery from the operation of the ointment, he took calomel 'till it produced a salivation; precisely the same taste was perceived, as when mercury was held in the mouth.

These experiments plainly prove, that the mercury entered the circulation, and was conveyed to the salivary glands.

Mercury has been found in the bones of persons who had taken it in large quantity, by Drs. Haller, Mead, Boerhaave, Brodbelt, Boyle, and many others.

Dr. Darwin detected the presence of nitre in the urine of a person to whom he had given it. He however attempts to shew, that it arrived there without entering the circulation, by a retrograde motion in the absorbents. I shall endeavour to combat this opinion in a subsequent part of this essay. I might now go on, and fill a volume with the enumeration of facts similar to the preceding; but, believing they are sufficient to prove that medicines are sometimes absorbed into the circulation in their active state, I shall omit them.

I know it has been said that the discovery of medicines in the secretions, does not prove that they entered the circulation in their active state, but that they were so changed by the absorbent vessels as not to be absorbed as medicines, but as the common matter of food, and were recomposed after leaving the circulation.

This assertion, in my opinion, has nothing to recommend it but the speculations of its patrons. It cannot possibly be true; because, it is impossible to diminish the active properties of some medicines which have been found in the secretions, by any change we are capable of making from their original state. Is it not notorious that mercury is least active in its fluid form, and is it not a fact that every change from this state encreases its activity? If then, every alteration in this mineral tends to render it more powerful, surely if the absorbents undertake to change it, they must produce a more active medicine. The theory, therefore, of the absorbent vessels possessing a power of changing and destroying the properties of medicines, is here directly opposed by an impossibility.

It has often been objected to the opinion I advocate, that, if medicines were taken into

the circulation in their active state, they would produce death; because, the most simple ones injected into the blood-vessels will cause this effect. Even admitting that they do produce death when injected into the blood-vessels (which, however, is contradicted by many late experiments) yet it only proves what is already known, that the internal surface of the blood-vessels is very excitable; that medicines suddenly introduced are too powerful for their excitability, and that to be beneficial, they must be carried there by the absorbents, and thus gradually introduced; by which means, the stimulus is proportioned to the quantity of excitability. No more occurs here than, *cateris paribus*, will happen when medicines are applied to other parts of the body. Thus, opium, if taken into the stomach in quantity suited to the degree of excitability, is an excellent medicine, but if over-proportioned is a deadly poison. So it is with injecting medicines into the blood-vessels; if adapted to the quantity of excitability, they may be beneficial, as I have before shewn, but if too powerful they will prostrate all action. It is therefore plain, that their operation is governed by the same laws, whether they be injected into the blood-

vessels, or whether they be applied to the stomach ; in either way, they may be rendered salutary remedies or fatal poisons.

Although I have attempted to prove, that medicines are sometimes absorbed into the circulation in their active state, and there produce their peculiar effects ; yet, as I have before observed, I believe they most frequently operate through the medium of the stomach. I shall, therefore, make a few remarks on this process.

The stomach, I consider as the most important part of the animal body. This is evinced by its almost universal presence in all animals. Many are found to want brain, heart, and other parts essential to human existence, but very few are without stomachs. The great excitability of this viscus, the unequal sympathy which it possesses with every other part of the body, and the servile dependence of the whole system upon it, has induced an eminent author to call it the seat of the soul. Doctor Rush, with his usual ingenuity, compares it to the house of representatives. It is certainly the spot where the systems meet, or are all represented—the truth of which is evinced, by different medicines producing their *specific* operation upon

each system, through the medium of this organ.—Every part of the body however, is not equally represented; but, I believe, according to their importance in the animal œconomy. Dr. Cullen asserts, that “there is a special consent between the stomach and the vessels on the surface of the body, so that the several states of these are mutually communicated to one another.”

By this “special consent” and mutual communication of states, he explains the sweating which attends vomiting. That the sympathy between the stomach and skin is great I agree, but that their states are not always alike, is proved by the fact, that sweating does not always attend vomiting, and that it depends entirely upon the medicine exhibited. Thus, if the vomiting is produced by squills or digitalis, it is accompanied by a copious flow of urine: if by the sulphate of mercury, with an increased secretion of saliva, and if by ipecacuanha or the tartrate of antimony, sweating. Here we plainly see that the Doctor’s opinion will not hold good, and that what he supposed to be the consequence of a “special consent,” is only the effect of the *specific* operation of medicines.

That medicines often operate through the

medium of the stomach is proved by the fact, that they have been vomited perfectly entire many days after having produced their salutary effects. The operation of the bark, in my experiment with it, was over before it could possibly have been absorbed. The effects of medicines are then, often so quickly produced in different parts, after being taken into the stomach, that no other way is left to account for their sudden manner of relieving, but on the principle of the representation of every part of the body in this important organ, and of the *specific* operation of medicines. It is in this way, we must suppose the peruvian bark exerts its beneficial influence, in preventing the accession of an intermittent paroxysm. It is in this way, that opium stops the paroxysm although the cold stage has commenced; and it is upon this principle that most medicines produce their salutary effects upon the human body.

In compliance with my promise, I shall now take up the consideration of Dr. Darwin's opinion which supposes, that, the absorbent vessels are capable of taking on a retrograde action, whereby their contents are regurgitated, and that, fluids often pass in this way from the stomach to the bladder.

Believing no such action or communication from one part to another ever to take place, I shall endeavour to shew, that the Doctor's theory has nothing to recommend it but his visionary speculations. In doing this, I shall briefly consider some facts which have been offered by other writers, and which would seem to evince its propriety; without being able to derive assistance from any person.

Dr. Darwin commences his observations by describing the Absorbent System; his description however is not an accurate one. Every reader must plainly perceive that his peculiar partiality for his favourite doctrine, leads him to attribute to these vessels, fewer valves than they really possess. This disposition is fully exemplified by his quoting authors, who wrote when little was known about this system, (because their assertions were more coincident with his theory) although they have been contradicted by late experiments. He observes, that “ many of
 “ the absorbent vessels, like some of the veins,
 “ are replete with valves—” “ These valves
 “ however, do not appear to be necessary to
 “ all the absorbents, any more than to all the
 “ veins, since they are not found to exist in

“ the absorbent system of fish, according to
 “ the discoveries of the late ingenious and
 “ much lamented Mr. Hewson!” Our au-
 thor discovers here what I am sorry to see—
 an improper construction of a writer’s words
 for the sake of establishing a favourite doc-
 trine. His anxious wish to bring the absor-
 bents on a level with the veins as to the num-
 ber and uniformity of valves, that he may
 hereafter derive some assistance from analogy,
 is very obvious—and, although he places
 them upon the same footing, yet he has no-
 thing to support him in so doing, but deduc-
 ing conclusions from the “ discoveries” of Mr.
 Hewson, which this writer’s own words will
 not warrant. The following are Mr. Hew-
 son’s words on this subject: “ These vessels
 (meaning lymphatics) in fish either have no
 “ valves or the valves readily give way, for
 “ it is an easy matter to fill them contrary
 “ to the course of the lymph.” These are the
 words from which Doctor Darwin so confi-
 dently concludes that the absorbents, like the
 veins are not always supplied with valves.
 Even supposing the absorbents in fish to be
 without valves, are we to infer that the lym-
 phatics in the human body also want them?
 Surely not—such an inference would be in

the highest degree absurd—because many experiments made before Doctor Darwin wrote, and with which he must have been acquainted, positively prove that every absorbent vessel in the human body is amply supplied with valves. But every impartial reader will agree with me in saying, that Mr. Hewson was not certain whether the valves were wanting, or whether they easily gave way, as he himself expresses.

Although this gentleman after his experiments could not determine whether there were valves or not, yet our author who did not see the experiments assures us, these “discoveries” prove that the absorbents like some of the veins are without valves!!! The plain and simple conclusion however, from Mr. Hewson’s own words is directly opposed to that which Doctor Darwin has unwarrantably drawn. The tender fabric of fish, necessarily implies the same structure of lymphatics—this plainly accounts for the ease with which the vessels were filled contrary to their valves. This conclusion is positively proved by the fact related by the ingenious Mr. Cruikshank, that the lymphatic vessels of all animals in which they have been found, are plentifully supplied with valves.—This au-

thor also remarks, that he should not easily believe any vessel to be a lymphatic, where the characteristic of valves was wanting; and he further adds, “ The vast number of valves
 “ not only distinguish and characterise the ab-
 “ sorbent vessels from all others in the human
 “ body, but also in quadrupeds, birds, amphi-
 “ bious animals, fish of warm blood, as they
 “ are styled, and even in fish of cold blood.”
 From experiments he says, that only some of the veins are supplied with valves, and even in those they are not near so numerous as in the absorbent vessels.

As these vessels are then distinguished from all others in several important particulars, no conclusions drawn from any appearances in other parts bear any relation to what may take place in them. If therefore Doctor Darwin brings forward (as we shall see) nothing but what he calls analogy in support of this doctrine, it must fall unsupported, as there is no analogy to the absorbents in the human body.

After finishing the anatomical part, the Doctor goes on to shew that “ the valves of
 “ the absorbent system may suffer their fluids
 “ to regurgitate in some diseases.” Upon the authority of Doctor Haller, he states that

mercury, fuel, or water, injected into the lymphatics, or air blown into them pass easily in a contrary direction to their valves, if the vessels are a little forcibly dilated.

Admitting the fact related by Doctor Haller, what does it prove? Certainly nothing in relation to the action of the absorbents in a living state. The striking difference between animals in a living and dead state is well known to Doctor Darwin, and that this fact relates only to the latter, is equally plain. To infer from it that fluids may regurgitate in a living state is equally unphilosophical, as to suppose the human system devoid of sensibility and excitability when alive, because it is so when dead. It only proves then what every man knows, that if the projectile force with which fluids are thrown into an inanimate tube be greater than the resistance offered by the inequality of its surface, they will pass through it.

I would here however ask the Doctor, how this quotation accords with his inference from Mr. Hewson's "discoveries"? Because injections passed through the lymphatics of fish, he concludes that they have no valves, and now attempts to show that they may easily pass through absorbents which are plen-

tifully supplied with valves. The cause of these peculiar assertions is obvious. By Mr. Hewson he wished to prove that the lymphatics are not always supplied with valves, by which means he would place the veins and absorbents on a level. This would very well suit his purpose—and by Doctor Haller he attempts to shew that valves are no great obstacles to the regurgitation of fluids. Yet to accomplish this, he is obliged to draw conclusions from the same facts which contradict each other!!!

Our author then relates an experiment which proves, that a bladder when inverted and filled with water, allows it rapidly to pass out; and from thence infers, “That there is
 “no obstacle, at the mouths of these vessels
 “(lymphatics) to prevent the regurgitation
 “of their fluids.” The same objection which were offered to the last experiment, apply with equal force to this, as well as to all others, upon dead parts. At any rate the conclusion which is drawn from it, must be viewed with astonishment, by all who have heard of Dr. Darwin’s fame. Mr. Cruikshank’s experiments plainly prove, that all absorbents are plentifully supplied with valves

to prevent a regurgitation of their contents.

Besides, in separating the bladder from the dead animal, a great number of blood-vessels are necessarily ruptured—the reasonable conclusion therefore is, that the water passed out through them.

The Doctor proceeds in his retrograde action, by offering what he calls analogies. He says the upper and lower orifices of the stomach are closed with valves, yet a regurgitation of its contents sometimes occurs. The colon is supplied with valves, which sometimes allows the same action—the like takes place in the lachrymal sack. And he quotes Dr. Haller who says, that a retrograde motion of the blood is observable in the veins of animals in the act of dying, from the very heart, to the extremity of the limbs.

I have already shewn that what may take place in any other part is no proof of the same occurring in the absorbent vessels, as they are characterised and distinguished from all others in the body. The orifices of the stomach have no valves although circular muscular fibres perform their functions by contraction and relaxation. As to the valve of the colon it is only one and that differs so

materially from the valves of the lymphatics that it is a matter of dispute whether it be a valve or only a sphincter.

The lachrymal sack has no valves, therefore a regurgitation of its fluid is by no means surprising.

I am sorry Dr. Haller does not mention the animal in which this retrograde motion of the blood is observable. I am however, induced to believe he has been entirely deceived. The deception arose I suppose from the following circumstance :

It is well known that the veins of animals in the act of dying become turgid with blood ; this appeared to the Doctor I suspect, to be the effect of a retrograde motion : but the fact is established that the heart and arteries retain life longer than any other parts of the body, and continue to contract until they become nearly empty, thereby filling the veins. As the heart and arteries then continue to act after the death of the veins, it is impossible for a regurgitation to take place in them as long as life remains. But even admitting the fact, it does not prove in the smallest degree that a retrograde motion ever takes place in the absorbents, because they are uniform-

ly and plentifully supplied with valves, and many of the veins have none.

After stating the foregoing arguments in favour of a retrograde motion in the lymphatics, the Doctor goes on to prove a communication from the stomach to the bladder by means of absorbent vessels, and that it is by this retrograde motion that fluids pass from the former to the latter. In support of this opinion, he first adduces the expedition with which cold water and other fluids when drank pass from the stomach to the bladder, and the similarity of the urine when produced in this hasty manner to the material that was drank. That, urine is very quickly formed after drinking, and that, it does resemble the fluid taken into the stomach is true; but does this prove that it gets there by a retrograde motion of the absorbents? By no means—the certainty of such a communication of vessels is far from being established, therefore conclusions drawn from thence are built only upon a supposed foundation.

No communication of lymphatics from the stomach to the bladder has ever been discovered in the human body. The only fact which would seem to favour this supposition is derived from Mr. Hewson who says, that

some small branches of lymphatics would “*seem*” to join those from the duodenum in birds. But Dr. Darwin has inferred from this observation, “anastomoses to be very frequent between the intestinal and urinary lymphatics as mentioned by Mr. Hewson.” How far the Doctor’s inference accords with Mr. Hewson’s remark I leave the reader to determine.

Even supposing the lymphatics of the stomach to join with those from the bladder, still fluids could not pass from one to the other in a retrograde direction ; because the fluids in leaving the stomach must go in the natural direction of the absorbents until they arrive to the communication with those from the bladder, when they must turn back and go in a retrograde way to get to this organ. That such a process never can take place in the human body must be obvious to every impartial reader ; for if there is a communication by means of lymphatic vessels between the stomach and the bladder, there must be the same between the stomach and many other parts of the body ; consequently fluids would as frequently pass to other parts as to the bladder.

The great quantity of blood which is sent to the kidneys in a given time, readily ex-

plains the early discharge of fluids after drinking. Doctor Monro observes “ the largeness of the renal vessels demonstrates “ that they cannot receive much less than an “ eighth part of the blood of the whole body “ at a time, and consequently above one thousand ounces of blood are conveyed through “ the kidneys in an hour, and it will appear “ but a moderate allowance for twenty or “ even fifty ounces of water to distill from “ that quantity of blood in the same time. “ Finally it is certain that both men and brute “ animals perish if the ureters are obstructed “ by a ligature or otherwise—we then observe also, that no urine can be found in “ the bladder.

The shortest time in which Doctor Darwin discovered the fluid in the urine after being taken into the stomach, was half an hour, and the quantity eighteen ounces. Now as five hundred ounces of blood pass through the kidneys in that space of time in common cases, as we know this may be increased by taking certain fluids into the stomach, and that the secreting action of the kidneys may also be much accelerated; that some fluids which Doctor Darwin mentions, require very little elaboration to be changed into urine,

and that, part of the eighteen ounces was secreted before the fluid entered the circulation—the process of the speedy production of urine after drinking, which the Doctor thinks cannot be explained in any other way but by retrograde motion in the absorbents, is easily discovered.

As to the substances detected in the urine by Doctor Darwin, it accords perfectly with the opinion I have endeavoured to establish, which is, that medicines are sometimes absorbed into the circulation in their active state, and consequently may be found in the secretions.

The Doctor then quotes the following facts from Doctor Haller, which however he did not see himself, but were observed by preceding writers. Doctor Haller mentions that Doctor Kratzenstein says he put ligatures upon the ureters of a dog, and yet he discharged urine. How much? The quantity is not mentioned; we may therefore conclude that it was very small, and that what was evacuated came its usual way, the ureters not being well secured. This conclusion is rendered undeniable by later experiments, wherein the ureters have been tied and no urine discharged.

I have already related what Doctor Monro, (whose authority is far more to be depended upon than that of such ancient writers) says upon this subject. Doctor Darwin takes care only to select such facts from Doctor Haller's collection, as bear the face of probability, but when they leap the bounds of possibility as many of them do, he passes over them in silence. To shew how far these facts are to be depended upon, I beg leave to introduce the following quotation from Doctor Haller. In doing this, I do not mean to insinuate that the Doctor himself is not to be believed, but that the facts which he has collected from writers who wrote when medical science consisted in witchcraft, charms, and the like, are not entitled to the least credit. He says upon the authority of others, when speaking of substances passing unchanged into the bladder, "*Aliqui etiam ad corpora varia respexerunt, quæ deglutita in vesicam descendissent, hordei spicas, petroselinum, herbas varias, brassicam rubram, anisi semen, pilulas, prunæ, aciculas, officula pilos, glandes plumbeas, vitri frustum, lolii spica.*" It is also said in the same book, that urine has continued to be discharged af-

ter the suppuration or total destruction of the kidneys.

Knowing how common it is for unusual occurrences to be exaggerated by those who discover them, (and particularly so if often communicated by different persons) and recollecting the source from which this fact is derived, we may safely conclude that the kidneys were only in a state of suppuration or partial destruction. Now that the urine should be discharged in this instance is nothing very strange or uncommon.

It only shews what may be seen every day, the transcendant wisdom of nature. Do we not often see the circulation of the blood in a limb almost entirely obstructed by the destruction of the principal artery, and yet this function performed by the small anastomosing vessels? Are there not cases wherein the brain has been nearly destroyed and yet its functions performed? Yes there are such instances upon record, and a friend of mine also informs me, he once saw a case where a patient was diseased for a long time, but the operation of his mind remained perfectly entire, at length he died, when upon opening the brain, at least three-fourths of it was destroyed. How often in consumptive patients are the

lungs found almost totally destroyed? And lastly, is it not a law of the animal œconomy that the gradual removal of a part of an organ should increase the power of the remainder. These considerations plainly evince that a part of the kidneys must have remained entire, which continued the secretion of urine.

As yet then we are furnished with no proof of a retrograde motion ever taking place in the absorbent system. Dr. Darwin however supposing the fact to be established, proceeds to account for many phenomena of diseases, by means of it. He asserts that the sudden diminution of the size of the abdomen in ascites attended with an increased discharge of urine proceeds from this retrograde action in the absorbents, and accounts for the absorption of fluids by the skin from the atmosphere in the same way!!—Now as there are absorbent vessels which arise from the skin and cavity of the abdomen, and which take up fluids from those parts and carry them into the circulation in their natural direction, to assert that they pass in a contrary way is offering violence to all human understanding.

The Doctor mentions several instances of what he calls translocation of matter from one

part to another which he says is performed by a retrograde action in the lymphatics. Even supposing matter to be carried from one part to another by the absorbents, surely the rational conclusion would be, that it passed in the proper direction of the valves, and not in a contrary way. But the fact is, that what he supposed to be the translation of matter, was nothing more than a morbid secretion of one gland, from the obstruction of another. This I shall render at least probable by answering the following question proposed by him: “Is
 “ it not an immutable law in animal bodies
 “ that each gland can secrete no other than its
 “ own proper fluids?” I answer no—it is not—and I can prove it by facts and observations. There are many instances of a stoppage of the gonorrhœal discharge from the urethra producing the same secretion from the glands about the eyes. There is a woman in this city who has had her menstrual secretion performed by her stomach for several years, and I have known a case, where this fluid has been secreted by the breast for two years. These are plain facts which evince that a gland may not only secrete a fluid in disease entirely different from what it secretes in health, but may take on the secretion of another gland.

Having now noticed the principal arguments offered by Dr. Darwin in favour of the retrograde action of the absorbents, I shall very briefly consider a case related by Dr. Senter of Rhode Island* which would seem to establish that theory. This case was in a young woman who was attacked with cough, difficulty of breathing and fever, which was succeeded by a suppression of urine and a discharge of pus from the stomach. This continued for three years during which time, if her urine was not drawn off by the catheter she frequently discharged a similar fluid from her stomach.

This with some similar cases would seem to evince a direct communication between the stomach and the bladder, but upon a minute examination we shall find, that the circumstance may be easily explained upon the known laws of the animal œconomy.

The explanation of the fact I suppose to be in this way; as the urine could not be discharged through the natural outlet from the bladder, the absorbents which arise from its internal surface were excited into action, whereby it was taken up and conveyed by them into the circulation. As it is necessary

* See Transactions of Coll. Phys. of Phil.

for the continuance of human life, that this fluid should be discharged from the blood, as it is a fact that if the action of one gland be obstructed, another by sympathizing with it will often perform its functions, and as the stomach is the great sympathiser in the body, the plain and proper inference is, that the fluid discharged from the stomach was a morbid secretion of that viscus. That the lymphatics of the bladder should absorb the urine in this case, is not an unusual circumstance, this takes place more or less always when the urine is long retained. If the duct which conveys the bile from the liver to the duodenum becomes obstructed, the bile is absorbed and jaundice produced.—That, the secretions of a patient labouring under jaundice, all partake of the nature of the bile cannot be denied, and that the same thing should occur when urine is absorbed in large quantities is the natural consequence.

I now conclude this imperfect Essay, and although I believe Doctor Darwin* has theo-

* The speculations which I have attempted to controvert, appeared originally under the signature of Charles Darwin, but as they are introduced into the *Zoonomia*, and are there adopted and supported, I have addressed myself particularly to the author of that work.

rized too far without the support of facts, yet I perfectly agree with him in the following observations: "There are some modern practitioners who declaim against medical theory in general, not considering that to think is to theorise; and that no one can direct a method of cure to a person labouring under disease, without thinking, that is, without theorizing: and happy therefore is the patient, whose physician possesses the best theory."

It remains now that I return my thanks to the Medical Professors of this University for the valuable instruction I have received from them. To Professor Barton, the American Linnæus, I beg leave in a particular manner to address myself, and to solicit an acceptance of my grateful acknowledgements.

Wishing each of you, Gentlemen, that honour and happiness which your indefatigable exertions in the diffusion of knowledge so justly claim, I bid you Adieu.

FINIS.

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